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Analyzing the Costs of Federal Research

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Criteria for Evaluating Research Projects and Portfolios

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Executive Summary

The ultimate social test of the information chartered by any research or information investor is its third party utility--the difference having the information makes in the behavior or decisions of users. This paper describes some prototype "strategies" and criteria for evaluating the utility of individual projects and "portfolios" of projects. Portfolio impacts should be the most significant factor in guiding an investor's resource allocation, ex ante, and in demonstrating prudent expenditures, ex post. Consequently, the paper discusses how to go about judging whether existing portfolios are the best that can be obtained.

The paper describes procedures for assessing three different modes of research: (1) basic research; (2) innovation research, aimed at developing new or improved products, services, or processes; and (3) applied research aimed at informing or affecting public decisions. Since research investors frequently involve themselves on the supply side of the research market as well as the demand side, the paper also describes best practice in evaluating science education and manpower training programs.

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Criteria for Evaluating Research Projects and Portfolios This paper argues that the evaluation craft develops by practice and application. Any free standing information "investor" has severe handicaps in ever knowing the value of the information it charters.[2] So an evaluation system of reasonable scale and scope is needed for setting priorities, ex ante, and for showing that resources were well spent, ex post. Congress can be considered an investor in Federal information investors, and so it has an interest in the evaluation systems that investors design.

1. The information investor's evaluation problem: Peer review in portfolio construction

Suppose that for m years some free standing information investor has been selecting or "purchasing" a portfolio of research projects in some field or discipline. Suppose, to be more precise, that the investor is a public agency charged with producing a public information good. So the information produced by the projects in its portfolio is supposed to benefit multiple users, not just the investor, at some future time $n > m$. Suppose that the investor has always had limited resources to construct the portfolio. At time t between m and n, the investor wants an estimate of the expected utilization at n of the information in his portfolio.

Through the years m, the investor's selection of projects for inclusion in its portfolio has been determined by their predicted or estimated quality as seen by contemporary

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[2]The R&D branches of mission agencies have internal users who specify demand ex ante and provide concrete tests of ex post worth. Thus, the operating branches of the Air Force set requirements for new aircraft and the R&D branches do the research necessary to meet the requirements. The test of the R&D is whether a flyable plane emerges with the desired characteristics. Free standing investors have no specific users, but produce a public information good for whomever can benefit.

research performers (reviewers) or by knowledgeable research managers aided by reviewers. Reviewers usually make judgments about the quality of a project without any direct comparisons of the alternatives facing the investor.[3] They judge the technical merit of a research design, the competence of the investigators, and the institutional infrastructure available for executing the proposed design. In each year up to m, projects are not explicitly chosen to achieve significant incremental gains in the expected information content of the portfolio, conditional on its current content. Projects are chosen on a one by one basis up to the point of resource exhaustion without reference to their incremental value to the portfolio. Ordinarily, reviewers know neither what is in a current portfolio nor the alternative projects that could be added to it.

In theory, the rapid feedback between information users and performers is supposed to act as an "invisible hand." At least for basic research, researchers and reviewers are supposed to adjust their activities so quickly that ex ante judgments about the salience and quality of any isolated single project remain congruent with developments at the frontiers of knowledge. In practice, the investor has no way of knowing whether this invisible hand is efficient, rapid, and has good discriminating power. There may be barriers and imperfections that prevent it from working well, but detailed knowledge about these barriers and imperfections will not be in the possession of the investor.

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[3]Some agencies use peer panels that rate multiple proposals, and so the panels can make direct comparisons of proposed work in their field. But they do not compare their findings with those of panels in other fields, since between-field information is held to be incommensurable.

Because the investor's ex ante selection of projects is not automatically geared to maximizing the expected informational contributions of project portfolios, ex post, the investor should assess the informational value of its entire portfolio in addition to evaluating individual projects. Not only do the individual projects in a portfolio have to provide results of high quality and relevance to users, but the portfolio itself should have the "right" composition, and, for rapid scientific or technological progress, the projects in the portfolio should be connected in an "optimal" way. In principle, portfolio evaluations should be used in setting relative investment priorities ex ante, since they provide a check on performance at the right level of budgetary aggregation. Congress, the OMB, and the heads of research funding agencies deal not in "retail," but wholesale, allocating resources by program, discipline or megaproject.

2. Research modes and strategies for evaluation

Basic research projects are generally easiest to assess. While the uptake or absorption of basic research by basic researchers is certainly influenced by the "culture" and rules of thumb prevalent in any given field, in general, it is technical quality and problem salience that determines usage.[4] In contrast, research designed to impact public sector decisions is the most difficult to assess. The reason is that, to be of any use at all, information for decision making has to have many more attributes than mere

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[4]For this reason, bibliometrics, the quantitative analysis of publications and citations--is such an attractive ex post evaluation technique. However, many decisionmakers suspect that bibliometrics true believers fail to adhere to canons of best practice, that is, remain ignorant of the citation norms of a given field, do not account for the number and prestige hierarchy of cited journals, do not account for the international specializations in different fields, etc. Without the qualitative, contextual information, bibliometrics thus levels out the field differences critical to prudent resource allocation.

technical quality. For example, it has to be timely, since its worth falls precipitately after a decision has been made or an action taken. Perfect information after a decision has less value than merely good information before a decision. But timely scientific research is hard to design and deliver.

Even when research for public decision making has all the necessary properties, it may not be used. There are many organizational and institutional barriers to doing so, and the barriers may have nothing to do with the quantity or quality of the research output. The worth of research then has to be determined by the potential improvements in outcome that could have been made by using the information, and such potential improvements are difficult to judge.

The evaluation of research to create new or improved products and processes (denoted innovation research) represents a middle case between basic research and research for decision making. The end objective of innovation research provides a way to measure the worth of information from research. Did the information contribute significantly to the innovation? Of course, the standard of contribution can vary. For example, a very strong standard would hold that the innovation could have been accomplished if and only if the research were accomplished beforehand. A weaker standard might hold that the innovation could have been produced eventually without the research but it would have arrived on the market much later.

Again, however, there are constraints. Even though information exists that could be used to produce and innovation, firms do not always have the internal capability to

recognize such information,[5] and not all innovations gain market acceptance, even if high quality information to create them has been made available by the research community. Thus, what has to be judged in many cases is the potential economic worth of information, not its actual worth in the market place as embodied in products, services, or processes.[6]

The rest of this paper sets up strategies or "algorithms" for evaluating the different modes of research discussed above: basic research; applied research for use in public decision making; and innovation research. Each strategy has been stripped to its core elements so that information investors can be clear about the requirements for each type.

3. The evaluation of basic research projects

Because of uncertainties attached to each and every evaluation procedure, the evaluation of basic research is best done by using ex post peer review and citation evidence jointly. The steps in an evaluation are as follows:

3.1 For any given portfolio, take a sufficiently large random sample of completed projects.[7] (The sample may be stratified so that information about subfields can be constructed.)

3.2 Design some kind of scoring system to be applied to projects.

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[5]See "W. M. Cohen and D. A. Levinthal, "Absorptive Capacity: Perspectives on Knowing and Innovation," Administrative Science Quarterly, vol. 35, 1990, pp. 18-152.

[6]Some information will never have any market value at all, but will have value to the firm. For example, research that shows a product or process is technically infeasible or too costly will save significant resources for a firm. Public resources might be saved as well were other firms to rush to imitate a technically flawed or overly costly product or process.

[7]The feasibility of doing this was established by the Department of Energy's review of its basic research projects in 1983

Scoring systems can be aggregated or disaggregated. In an aggregated scoring system, evaluators make an overall summative judgment about completed projects the way they do, for example, in the NSF ex ante peer review system. NSF reviewers assess the total quality of a project on a five point scale ranging from excellent to poor. The warrants for this overall rating are nominally elicited from general, unordered qualitative comment on the proposal. Ex post, the same kind of scale and warrants can be used, but applied to research product.[8]

Alternatively, since aggregate scales do not clearly include dimensions an investor might find important--for example, efficiency in information production--an investor, ex post, might want to use multiple measures of project outcomes. For example, the following seven measures might be applied:

(1) Value of the information produced: salience, relevance, importance to the field.
(2) Probability of use
(3) Originality and imaginativeness of results
(4) Efficiency and cost: whether more information could have been gained at the same cost.[9]
(5) Training and education impacts
(6) Impacts on infrastructure and capability to carry out additional research in the future
(7) Overall Merit

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[8]See, for example, R. N. Kostoff, "Evaluation of Proposed and Existing Accelerated Research Programs of the Office of Naval Research," IEEE Transactions on Engineering Management, vol. 35, 1988, pp. 271-279.

[9]In judging efficiency ex post there will be a "hindsight" problem. After a project has been completed, cheaper, alternative ways to generate the results will be obvious.

If the investor wants quantitative analysis, each criterion can be assigned a numerical scale, and the criteria themselves can be weighted in different ways according to their importance to the investor. The total merit of a project can be defined as the sum of the scores on the individual criteria, although this quantification should be seen as a way of making qualitative judgments clearer, not as a substitute for such judgments. In addition, some global evaluation might be made, where the value of the whole was more or less than the sum of its parts. However, scores assigned to particular dimensions of the research product provide consistency checks on unanchored summative evaluations.

3.3 The overall ex post peer review evaluation of particular projects should then be compared with any associated bibliometric information. In doing, so adjustments may have to be made according to a project's "vintage" or age, since a "young" project might not have attracted many citations, despite obvious technical quality, and the results from an "old" one might have been so assimilated that users no longer cite it.[10] But these are technical matters.

If ex post peer review and citation evidence both indicate the same quality for a project, then the investor can have greater confidence that the project is, in fact, of that quality. If the two measures are not congruent, then the project needs to be subjected to more intense analysis to explain the discrepancy.

4. Evaluation of a basic research portfolio

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[10]In addition, adjustments may have to be made for critical or negative citations. However, some science currently judged as not meritorious will turn out to be so and vice versa. Thus, evaluations should be revisited from time to time.

Since ex ante reviews are estimates or predictions of expected information output, and research outcomes are highly uncertain, a portfolio, ex post, will ordinarily contain some projects of high overall estimated quality, some of medium quality, and some of low quality.[11] Some projects originally predicted to be of high quality will turn out to provide low quality results and vice versa. Some projects that were originally entered into the portfolio should have been rejected in hindsight. Some projects that were originally rejected probably could have delivered reasonable quality. But we can make no direct estimate of the ex post quality of rejected projects. However, by examining samples of rejected projects funded by others, we may obtain some notion of the imperfections in an investor's selection process that led to unwarranted rejection.[12]

The rational investor wants to know if his entire portfolio is reasonable relative to some other portfolio that could have been supported. The first thing to do is characterize the actual, purchased portfolio. We can do this, for example, by aggregating the project statistics. We can compute the total citations and/or co-citations that any portfolio has received. And we can derive the mean quality and variance of the portfolio as determined by ex post peer review of the projects in it. Distributions by subject matter, equipment intensity, and other characteristics can also be established.

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[11]The scientific community's rhetoric about peer review is that is invariably accurate, quite unlike most other predictions or forecasts of future events. Since there is little continuous evaluation of the predictive powers of the various peer review systems in use, the rhetoric is hard to judge.

[12]In the federal grant system this is almost never done. Most agencies do not track the funding path of their rejected proposals. Indeed, few maintain records at all on rejected proposals other than date of submission.

Given descriptions of the actual purchased portfolio, the investor needs to make comparisons between it and one that can serve as a standard or norm. Where the missions and priorities of investors overlap, then the actual portfolio can be compared to similar ones constructed by competing investors. Alternatively, the investor can submit the actual portfolio to experts working at the research frontiers to see whether its structure and content correspond to one, that in the minds of the experts, would make a significant contribution to its field.

In still another approach, evaluators may be able to construct an explicit "ideal" portfolio such that the end results from the actual one could have been obtained earlier or more cheaply. The deviation of the actual portfolio from the ideal one in substantive content, research methods, labor or capital intensity or other salient characteristics is a measure of the worth of the actual and offers guidance on future additions to the actual. The question in such a counterfactual comparison is whether the investor could reasonably have foreseen during the years m--with the information then in hand--that some alternative portfolio would have greater worth (be closer to the ideal) and whether the investor could have overcome selection barriers or imperfections that led him to choose the less-than-ideal portfolio he did. If such foresight were possible, then the investor can now be more alert to the same barriers and imperfections in adding to portfolios.

7. Evaluating innovation research

Once an innovation has gained market acceptance, the actual research behind it can be "traced" to see if it was critical to the innovation, although any particular sequence of

critical research events may not be unique.[13] Then we can see whether significant amounts of the critical research appears in the investor's portfolio. Given market acceptance, we can also estimate the economic worth of the R&D that led up to the innovation and compute both private and social rates of return, ex post.

At t, the time of evaluation, however, an intended innovation may not yet be in hand. If so, then evaluation procedures can be similar to those in the previous case. Select a sufficiently large sample of projects from the portfolio, assemble a mixed team of reviewers, and apply a series of reasonable criteria to the sample. For a public investor, what has to be judged are the factors that led to support of the research in the first place--presumably relatively high social returns, but private returns too low to induce investment in the research. But now at t there is more information.

Representative criteria here are:

(1) The current expected private rate-of-return and its magnitude: Is the expected private rate still too low to induce private completion of the research activity.

From a strict welfare economics perspective, a public investor should undertake research to benefit the private sector when and only when there are significant expected social returns, but the estimated private rate at the beginning is too low to induce the necessary research by private innovators. Now at time t, the prospective private sector innovator and the public investor both have more information about the expected

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[13]See E. Mansfield, "The Social Rate of Return from Academic Research," unpublished paper, 1989.

returns. In fact, the investor's public resources have reduced the private cost of innovation, raising the private rate-of-return. The expected private rate-of-return may have risen to the point where the prospective innovator should carry on the research itself. In fact, the prospective innovator may wish the investor to desist, since the continued use of public funds will constrain the appropriability of future returns.

A private sector innovator, of course, may have an incentive to estimate his own returns as low, even if he now thinks they will be high. For the low estimate may keep public funds flowing. However, the innovator has to balance the value of additional public funds against the decrease in appropriability.

(2) The current expected social rate-of-return: Is the expected social return still greater than the private return and greater than the one that can now be earned on alternative public investments now available.

From a strictly economic perspective, the sunk costs of prior investments are not supposed to matter. Consequently, the question is whether additional public funds at t should be invested to support the innovation. But this depends on whether competing investments at t promise greater social returns. In practice, sunk costs matter a lot to public investors. So if innovation research promises any social return at all, it will probably be continued.[14]

(3) Current probability of technical success, if additional resources are invested

(4) Current probability of market success, given technical success.

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[14]Bureaucratic incentives being what they are it is likely that support would be continued even when social returns are negative. See H. Averch, Private Markets and Public Intervention (Pittsburgh, Pittsburgh University Press, 1990), Chapter 5.

There is no exact equivalent of citations for innovation research, although citation methods have sometimes been applied to innovation research. Some portfolios of innovation research may produce patents or licenses that can be counted or whose value can be estimated.[15] For example, such counts have been done for pharmaceuticals.

Given an actual innovation, one could again try to construct an ideal portfolio--that is, one that contained all the information leading to the innovation--and then check to see how much of the required information was in the investor's actual portfolio. Alternatively, the investor could submit his actual portfolio to experts knowledgeable about the potential worth of innovation in a given market and obtain qualitative judgments about it.

The investor could, of course, choose to postpone evaluation to the end stage n where market acceptance would be known. The investor would have to be willing to trade the complete certainty about merit provided by a market test for the time and money involved in continuing to fund its portfolio without "pruning" and reallocations based on evaluation.[16]

6. Evaluating projects and portfolios to improve public decisionmaking

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[15]See W. B. Ashton and R. K. Sen, "Understanding Technology Change Using Patent Information," 1986, unpublished paper and K. Pavitt, "Patent Statistics as Indicators of Innovative Activities," Scientometrics, vol. 7, 1985, pp. 77-99.

[16]"Pruning" of projects and even whole portfolios is a common practice in the private sector and one for which managers are frequently rewarded. Quite the reverse holds for public sector managers.

Suppose that an investor has constructed a portfolio of applied research projects whose purpose is improving public decisionmaking. Information is, of course only one component of such decisionmaking, and other components may be of far more importance. To have any impact at all, information for decision making has to have significantly more qualities than basic research information. The evaluation of research projects aimed at improving public sector performance can proceed in terms of whether they have the necessary properties. Some typical qualities are:[17]

(1) Contingent accuracy or validity: Information should be at least provisionally true according to current scientific canons.

(2) Timeliness: Limited information at the time of decision is of far more value than the most relevant, highest quality information after a decision.

(3) Controllability: Information should be related to instruments and actions controlled by decision makers, i.e. manipulable variables.[18]

(4) Implementability: The research should provide actions or policies that can gain acceptance by those that will have to carry them out. In the public sector, research must be acceptable to the bureaucracies expected to produce products or services derived from research.

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[17]We should, of course, recognize that decisionmaking is a process that contains much "slack." Decisions may not have to be made when decisionmakers say they "must" be made; they may be reversible and corrigible; they may reappear to be solved again. Thus, scientists and analysts should be thoughtful about rushing to meet a timeliness criterion by compromising accuracy or quality of research.

[18]The "rational expectations" literature has suggested that decisionmakers will have great difficulty manipulating policy variables because the affected parties anticipate the manipulation and adjust their behavior to offset it.

Given these kinds of criteria, the evaluation of public sector research projects can be accomplished through a broadened, mixed ex post peer review.[19] Researchers and decision makers have to assess projects and portfolios jointly using the criteria above or some other reasonable set. Best practice again suggests projects and portfolios be assessed with more than one procedure. There is, however, no exact counterpart of citation analysis, since most public policy and management research that is actually used will not appear in refereed journals. The impacts of some internal agency research may only become known years later.

An agency can compare its public sector research portfolio to competing portfolios. In the private sector, firms have some notion of what is in their rivals' portfolios. At least, to find out they spend significant sums on industrial intelligence. In the public sector, stakeholders in a decision frequently carry out their own research and construct their own portfolios, and these can be compared and contrasted.

A direct test can be made by interviewing cognizant decision makers about the information they use in their decisions, or could use, if it were available in the right form. An existing portfolio can then be compared to the portfolio decision makers would have preferred, although decision makers are usually not clear or right about the information they need.[20] One of the functions of free standing research agencies,

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[19]This suggestion goes all the way back to various reports on scientific and technical information produced in the 1960's. See H. Averch, A Strategic Analysis of Science and Technology Policy (Baltimore, Johns Hopkins University Press, 1985), Chapter 5.

[20]It is characteristic of public and (private) decision makers to ask for more information than they have. However, once they get additional information they do not use it. But then they ask for more information. See M.S. Feldman and J.G. March, "Information as Signal

compared to in-house agencies, is to raise questions about the information decision makers are currently using and to find substitutes or additions that would be superior.

The worth of a portfolio can then be estimated in terms of whether it improved the definition and perceptions of a problem, provided surprising, but feasible ways to attack the problem, or created a richer menu of options.

8. The evaluation of science education and manpower programs

Some R&D investors believe that, without assistance, the market for scientific and technical human capital will produce too few practitioners overall, too few of sufficient "quality," or too few relative to the always very large social and economic gains from doing more science and engineering. Consequently, they undertake supply side educational and training programs in addition to underwriting the production of information.

The evaluation of education and training programs has a long history and a very extensive literature. Best practice involves building in an evaluation component at the beginning of such programs. For example, in the case of some new science curriculum, the development of the curriculum may involve randomized testing of the prospective users. If, say, high school students were the expected users, then some randomly selected students would be taught from the new curriculum (treatment group), and others would be taught from the old one (control group).

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and Symbol," Administrative Science Quarterly,vol. 26, 1981, pp. 171-186; J.G. March and J.P. Olsen, Rediscovering Institutions: The Organizational Basis of Politics (New York, The Free Press, 1990.)

Subsequently, both the treatment group and the control group would be given appropriately designed tests of cognitive and affective performance. Test scores would then be compared to see if there were significant difference in achievement or cognition that could be attributed to the new curriculum. For example, NSF curriculum development projects have been evaluated in terms of achievement, perception and attitudes, lab and analytical skills, creativity, etc.

If some curriculum becomes a national one and competes with other national ones, then its performance can be measured through statistical means. For example, individual student performance and the average performance of schools are frequently regressed on curriculum type, student and school characteristics, and social and economic factors. The coefficients of the curriculum exposure variables give the net incremental impact of the new curriculum.

If long term effects--such as increasing the probability of selecting a science or engineering career--were the objective of some new curriculum, then some sample of students would be followed up over time to see if exposure increases the likelihood of choosing such a career, holding other personal and social factors constraint.

Manpower training programs have a similar evaluation tradition. Employability, job mobility, and expected current and lifetime earnings have all been related back to exposure to training programs. Individuals and groups have been frequently followed up to see how their histories have been affected by program exposure.

Evaluation designs for education and training programs, should be successfully arranged so that some results are available whenever there is a demand for them.

Earlier periods, of course, have much less certain information about results. But evaluation designs should generate some information when it is needed. If an investor wants to improve his ongoing education or training programs, he cannot wait, since the production of scientists and engineers takes a long time. If he wants evidence about stopping or continuing, he also cannot afford to wait. Waiting involves additional sunk costs that will make it ever harder for him to stop, even if stopping is warranted.

9. The craft of evaluation and research priorities

The evaluation of research portfolios is itself an art or a craft. Like any craft, skill depends on practice and application. Any rational information investor should be willing to practice this craft. The major problem facing any investor is setting priorities, ex ante, and allocating scarce resources in relation to priorities. Setting priorities via the intuitive, interested judgments of stakeholders can involve significant errors in overall allocation. For given amounts of public resources, some other portfolios not selected, may give us far more information. Ex post evaluation provides checks on the intuitive judgments. And this check, with care in design, can be obtained at reasonable cost.

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Criteria and Rules for Evaluating Competing Megaprojects

As the body of scientific knowledge grows the number of proposed megaprojects--large scale, complex, costly, long term projects can be expected to increase. First for any given stock of knowledge, additional increments will usually require more sophisticated, costly, and larger instrumentation and infrastructure. Second, technological change and supply push from the instrumentation industry creates the potential to formulate and attack new questions never before addressed. Third, given concerns about technological competitiveness and the desires of national stakeholders to maintain their positions, the number of proposals emanating from the research community that seek to translate basic research knowledge into technological demonstration and development megaprojects should also be expected to increase.[1]

Origins of the Problem

Until recently megaprojects were not common in the conduct of research or considered necessary for scientific or technical progress. Issues of public investment in megaprojects were handled on a case by case basis. Projects were not considered to compete against each other for scarce resources. For example, no one proposed reducing the scale of Project Apollo so as to release funds for work on the SST or the Tevatron facility at Weston, Ill. In the 1980's and 1990's, however, the number of proposed megaprojects coming from the scientific or engineering communities have increased significantly. Among these we might include the Superconducting Supercollider, the Space Station, the Moon-Mars trip,

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[1]Nicholas Rescher argues that the further and further we get from direct sense experience, the more costly and complex research technologies we need for progress. See N. Rescher, Scientific Progress: A Philosophical Essay on the Economics of Research in Natural Science. (Pittsburgh, PA: Pittsburgh University Press, 1978).

the hypersonic aircraft, earth observing system, the Strategic Defense Initiative (SDI), and the Human Genome project.[2] The original estimates for these seven projects alone totaled $528 billion (net of inflation). The current estimated cost for the same project is about $580 billion.[3]

What policy problems, then, do megaprojects pose for the United States. Whether or not megaprojects should compete against one another for scarce resources is an open question. From one perspective, each one is nominally related to some agency mission. To the extent budgeting is rational for an agency, its megaprojects should compete against other missions and programs. NASA, for example, is the source for several space megaprojects, and rational budgeting would force its administrators to make choices between them. However, from a national perspective, while there will always be limited resources for R&D megaprojects, they are part

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[2]That these are *big* projects is not in dispute; whether or not some are *science* projects is. This report has already referred to the public works and earmarking dimensions of big science projects. Among other lists of such competing projects is that of the Industrial Research Institute (IRI) which had a sample of its members rank currently proposed megaprojects. The Human Genome ranked first. Then, in order, came the hypersonic aircraft, the space station, SDI, and the SSC. IRI appears to be weighing social benefits more heavily than scientific ones. See Research-Technology Management, vol.33, March-April 1990, p. 5.

[3]The Chronicle of Higher Education, September 12, 1990, p. A28, reports original and current cost estimates submitted to Congress just for the R&D investments required for the 7 projects (most not adjusted for inflation). High technology R&D project cost estimates are notoriously, but unsurprisingly, low. For example, a number of Rand Corporation studies shows the underestimation in the actual costs to develop high tech military aircraft. R&D megaprojects have even greater uncertainties than advanced military aircraft, because their lack of precedent implies one has to learn by doing. There will always be technical and economic surprises. Project histories suggest that few of these are cost-reducing. For some of these histories, see O. P. Kharbanda and E. A. Stallworthy, How to Learn from Project Disasters: True Life Stories with a Moral for Management (London: Gower, 1983) and P. W. G. Morris and G. H. Hough, The Anatomy of Major Projects: A Study of the Reality of Project Management (New York: John Wiley and Sons, 1987).

language of economics, these benefits and costs can be discounted, and the net present value of the dam can be computed. If the net present value of the dam is positive, one concludes the investment is warranted.[8]

Furthermore, if a new dam competes against a new highway for the same dollars, the benefits and costs can be made commensurable. Compare the net present value in dollars accruing from investing in the highway as opposed to investing in the dam. If the net present value of the dam and highway projects are both positive, but the dam's is greater, then one should invest in the dam.

What is the Megaproject Problem?

Relative to the problem of choosing between dams and highways, the megaproject problem actually has two different facets. The first is: For a given total R&D budget, what should the distribution be between "small science" and megaprojects?

Small science projects, collectively, are said to produce more information than megaprojects and to provide more downstream social and economic benefits. However, some megaprojects are necessary for progress in some fields. Following standard economic prescriptions, one should invest in small science and megaprojects until the incremental gain in information per dollar of investment in each mode is equalized. however,

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[8]The benefits, of course, often go to different groups and organizations. Costs are also distributed differentially. This is inevitable. However, these distribution questions are supposed to be made transparent via explicit weighting schemes rather than through practical, but tacit political deals.

ex ante no one knows where the point of equality will be.[9] Within a field, one can make estimates of the information to be gained from a megaproject as opposed to small science. Thus, high energy physicists have decided they prefer their SSC megaproject to the equivalent amount invested in smaller, less costly high energy physics.[10]

The second problem is: Given excess demand for investments in megaprojects, which ones should be selected. The discussion here concentrates on the second problem. In the case of competing megaprojects, the social and scientific benefits (information) that will be derived from investing in one are incommensurable with the information that would be derived from investing in some other.[11] Information derived from a space station, a trip to Mars, or an SSC will not in all likelihood

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[9]Since megaprojects, by definition, cannot be divided up into small pieces, one might not be able to achieve the exact allocation where incremental information per dollar of public expenditure is equal. But one might try to minimize the difference between the additional information per dollar gained from megaprojects and that obtained from small science expenditures.

[10]This must be an inference, since the scientific community ordinarily does not elicit preferences through a budget-constrained process. It is, of course, in the interests of SSC proponents to argue that there is no trade-off, but that the SSC simply adds resources to the total available for high energy physics. Research technologies may also preempt any tradeoff between small and large science. Thus, high energy physicists claim that the only way to address their most interesting questions is with an SSC or something even larger. Low energy and solid state physicists argue that they can produce more scientific and social benefits at <u>less</u> cost.

[11]Establishing social benefits is difficult, because the information would have to be produced, translated into tangible products and services, and then be used. But there may be institutional barriers that prevent the social uptake of highly valuable information. So we are left to judge the <u>potential</u> worth of information from megaprojects.

help decipher the human genome and vice versa.[12] Thus, standard rules for allocating fixed funds among different public investments are very difficult to apply. So the question is: Given incommensurable streams of information (benefits) can we find rational ways of making our investments in megaprojects?

Even with incommensurable information, if research technologies for each different type of megaproject permitted information output on a continuous basis, there would, of course, be no problem. Every megaproject could be broken down into a series of small science projects. Research sponsors could choose the level of information they preferred or that was consistent with some budget constraint. With continuous information flows, they could even establish the marginal benefits of additional investments in each megaproject. For example, they could use a rule that said we fund up to the point where information returns started to fall, and we could ask experts on each megaproject when that was starting to happen.[13]

If information returns are rising, they should invest more, and if falling, invest less. Assuming diminishing returns to additional dollars, if they were to continue doing this, they would eventually approximate the economist's rule. For non-lumpy inputs and outputs, and a fixed investment, economics suggests that we allocate until the marginal returns

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[12]If space station advocates believed they could co-opt genome advocates, we can be fairly certain that they would argue that some genome experiments might be particularly illuminating if done on the station.

[13]Experts on any given megaproject will have incentives to inflate their estimates, since they do not face the discipline of a competitive market that would penalize bad estimates. However, the process of eliciting estimates can be structured to simulate market-derived estimates.

are equal, that is, where the incremental information per dollar of expenditure are equal.

We will, of course, never know when we achieve this condition, but we can ask whether information returns are falling or rising. If rising, we should invest more, and if falling, we should invest less. Assuming diminishing returns to additional dollars, if we keep on doing this, we will eventually approximate the economist's rule[14]

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[14]See, for example, J. E. Sigel, J. D. Graham and M.A. Stoto, "Allocating Resources Among AIDS Research Strategies," Policy Sciences, vol. 23, 1990, pp. 1-23. The authors asked seventeen nationally ranked AIDS experts to estimate the marginal or incremental value of additional funds for different AIDS research investments in terms of some pre-specified social outcomes. The information that would be gained from these different investments is incommensurable, but their expected contribution to the pre-specified social outcome lets them be ordered. The megaproject problem, however, is more like judging investments in AIDS vs. heart disease vs. cancer.

How Are Megaprojects Now Chosen?

The Federal government has no national program budget for megaprojects, although the current demand is estimated to be about $60 billion.[15] Megaproject proposals arise from routine budgetary processes in each R&D agency.[16] In general, the agencies use a "requirements" approach. The knowledge that can be gained from each "vintage" or generation of capital inputs becomes exhausted, and scientists and engineers propose to develop new vintages that will push the state-of-the-art. Ordinarily, they will not be forced to propose vintages that will be cost-efficient. Once the "requirement" has been set, it will then be costed out. There is no guarantee that the hardware and software inputs to meet the requirement will be least cost, since no budget constraints are not commonly used in deriving the requirement. Thus, NASA developed shuttle technology when there were other alternatives available that would have pushed the state-of-the-art for less money. Since standard R&D budgeting does not force anyone to confront the issue of additional information for additional dollars, agencies will select megaprojects that serve their intrinsic (and extrinsic) bureaucratic interests.

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[15]See "Big Science: Is it Worth the Price," New York Times, May 27, 1990, I, 1:1 and Jurfe 5, C, 1:2.

[16]If the government had a capital budget distinct from its operating budget, then it could consider megaprojects against each other and against other capital investments. Many countries, for example, Canada, West Germany, and Sweden, have capital budgets for all functions, including R&D.

Since there is excess total demand for megaprojects, the actual number the United States chooses is determined politically, in the Congress. Research elites in the science and technology community believe there is something inherently wrong with political allocations, because such allocations are inferior to those that would be selected by peer review processes.[17] Peer review processes currently in use tend to break down when confronted with incommensurable information flows from competing disciplines, fields, or projects.[18] Cross-field or cross-discipline allocations are rarely ever accomplished by peer review, since expert reviewers in one field or discipline are very reluctant to judge the scientific or technical merits of information from other fields.[19] Cross-field and cross-discipline allocations, in reality, are set by a tacit political, bargaining, and lobbying process that goes on inside the R&D

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[17]See, however, D. Wittman, "Why Democracies Produce Efficient Results," Journal of Political Economy, vol. 97, 1989, pp. 1395-1424. Wittman shows that when political markets are working well, they achieve efficient allocations.

[18]Scientists may be able to define a peer review process that does the job, but (as discussed in Chapter 3) they have resisted doing so. Recall that in his 1988 Presidential address to the National Academy of Sciences, Frank Press called for the science and technology community to establish priorities among scientific and technical research, education, and megaprojects. He went on to give his own priorities. A number of members of the community criticized Press for suggesting this and the rest ignored his challenge. In 19899, Press abandoned this tack and called for greater Federal investment in R&D. Taken together, these two are powerful messages delivered to the political and scientific communities. See "NAS President Frank Press calls on science to set own priorities," Chemical and Engineering News, vol. 66, June 13, 1988, pp. 27-30.

[19] There are no rules inside the scientific enterprise that suggest that one kind of information is superior to another. The existence of such rules would imply that information from different fields could be made commensurable. See H. Brooks, The Government of Science Cambridge, Ma.: MIT Press, 1968).

funding agencies. This hidden political selection process inside the R&D agencies (see box 4-X bxflamit). This hidden selection process is not, on its face, clearly superior to a wide open, competitive one in Congress

How Could Megaprojects Be Chosen?

The choice of megaprojects must always be accomplished through some political process. The question is whether we can improve the political process by injecting some a priori, "rational" analysis. Below are some relatively low cost, rational approaches that could improve the process.

Rule 4.1: Force consideration of cost as well as "requirements" at the very earliest stages of choice.

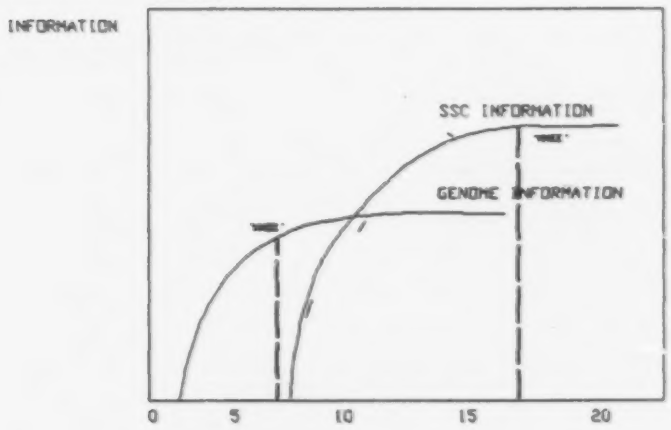
The core R&D budget process in the agencies could be modified so that working level staff and their research performing clients always consider "requirements" in conjunction with expected cost. They could be forced to think through whether their requirement or need could be met at a lower cost with alternative research technologies. Natural trajectories of discovery may dictate that only one research technology can generate the information required, but this seems unlikely. Even some of the information to be gained from the SSC can probably be gained with other technologies, if not so elegantly or as quickly.[20] Without cost

[20]Alternatively, staff can be asked to think through how much of a requirement they can meet at alternative costs. On the other hand, if the requirement is information about Mars, then there are ways to acquire some of it without going there. On the other hand, if the requirement is setting up a base on Mars or settling Mars, there is no substitute for going there. However, such requirements are more those derived from the state of our political economy than of science and would have to be evaluated as such.

containment efforts early, expensive technologies will quickly become entrenched or locked in.[21]

Rule 4.2 For competing megaprojects, estimate the budget levels where information returns will start to diminish.[22]

Suppose that the only two megaprojects before us are the SSC and the Genome project. Figure 4-1 shows hypothetical information patterns from the SSC and the Human Genome project under a diminishing returns assumption.



[21]See P. Schulman, Large Scale Policy Making (New York: Elsevier, North Holland, 1980).

[22]According to the scientific community, science is never subject to diminishing informational returns, because each new discovery makes possible further novel discoveries. While this may be true, it appears empirically that each succeeding discovery costs more to obtain. The argument is that deeper and deeper discoveries take us far away from our own direct experience and thus require more elaborate and expensive technologies of discovery.

Notice that there is no return at all before a 7.5 billion investment in the SSC and about $1.5 billion in the Genome.[23] Then it would be reasonable to set the total megaproject budget at approximately $25 billion, the sum of the budgets at the "knee" of each information curve. The information remains incommensurable, but we can set the level of total investment as long as we known whether incremental information returns for each project are going up, down, or remain the same.

> *Rule 4.3 Where multiple research technologies are available to generate desired information, start more than one. Track and monitor costs and identify and reduce the uncertainties.*
>
> *Rule 4.3.1: If a go decision turns out to be appropriate, select an alternative that gives lower costs or less uncertainty based on actual operating experience.*

Although Rule 4.3 may sound like waste and duplication to Congress, it is a straight forward application of parallel R&D for large hardware systems. A "fly before you buy" approach is an inexpensive way to get more information about necessarily complex systems. Modern R&D megaprojects look and behave more and more like other large scale technological systems. So costs will tend to be underestimated, and unavoidable complexity will generate error.[24]

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[23]The total estimated cost of the Genome is said to be about $4 billion. Some argue that we can get significant social benefits by mapping about one-third of the total genome. For illustration, mapping one third of the genome is assumed to cost about 1.5 billion, slightly more than one third of the estimated cost.

[24]The difficulties with the Hubble telescope have strong analogies to our difficulties in deploying complex military hardware like the B-1 and B-2 bombers within estimated costs and without performance deficiencies.

Rule 4.4: Once a given set of megaprojects has been selected, carry out real time, formative evaluations and give rapid feedback to those involved.[25]

Megaprojects will involve considerable learning by doing, and it is important to keep track of what is being learned. If real time evaluation had been built into the construction of the Hubble telescope, the evaluators could have pointed out the worth of a full scale test of the mirror relative to the costs of error. While there is no guarantee that operators of megaprojects will listen to evaluators, the latter become another constituency that needs to be satisfied.

All the previous rules require expert judgment and predictions. Experts may express themselves only once on the ex ante cost-benefits of a megaproject. So we have:

Rule 4.4: Take predictive track record into account when selecting experts.

Any portfolio of megaprojects will be high cost despite the most efficacious use of Rules 4.1-4.4. Consequently, we have:

Rule 4.5: Megaprojects that can be cost-shared internationally should be preferred to those that cannot be.

This means megaproject output is conceived as a contribution to world science and that the information is to be appropriable by all who want it and can benefit. Thus, Rule 4.5 implies that there are few private benefits to be gained by the community that operates a given megaproject. The SSC would not be defined as a competitor of CERN in some private

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[25]See K. Guy and L. Georghiou, "Real-Time Evaluation and the Management of Mission-Oriented Research: The Evaluation of the Alvey Program: Aims, Achievements and Lessons, 1989, unpublished paper, presented to the ECE Seminar on Evaluation in the Management of R and D, April 3-7, 1989.

particle race pursued by American high energy physicists; there would also be no national objective to keep the rate of discovery by U.S. high energy physicists above that of European or Japanese physicists. Despite claims that discovery races are necessary to keep discovery rates high, cost-sharing and information sharing are consistent with a view of basic research as an appropriable, world public good.[26]

Conclusions: Improving the Process of Megaproject Selection

This section has defined the megaproject selection problem, discussed how it arises, and suggested some rule for improving the political decision process involved in choosing between them. Any and all ex ante estimates about the costs and benefits of megaprojects depend on the judgments and forecasts of experts, acting individually or collectively. There is no way around this, but there is value in knowing the prior predictive track record of the contending experts.[27]

Megaprojects will always be selected through a political, public process because of scale, lumpiness, and incommensurability,. Standard Disciplinary peer review will hardly suffice, and every scientist and engineer knows that the big tradeoffs in any R&D agency are made politically. The question is whether political processes can be improved

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[26]There are two relevant literatures here, not unrelated. One is analysis of "simultaneous multiple discoveries" and creativity in science; the other is on data sharing. On the former, see D. K. Simonton, Scientific Genius: A Psychology of Science (Cambridge, UK: Cambridge University Press, 1988); on the latter, see D. S. Cordray et al., "Sharing Research Data: With Whom, When and How Much?" presented at the Public Health Service Workshop on Data Management in Biomedical Research, Chevy Chase, MD, Apr. 25-26, 1990; and E. Marshall, "Data Sharing: A Declining Ethic?" Science, vol. 248, May 25, 1990, pp. 952, 954, 955, 957.

[27]The problem of choosing sound experts is analogous to the problem of choosing good political or economic advisers. See H. Goldhamer, The Adviser (New York: Elsevier, 1977).

at the margin. The rules suggested above do not require elaborate new budgeting schemes for megaprojects, but may force decision makers, sponsors, and researchers to be more conscious of what they are doing when they generate megaprojects or endorse them.[28]

[28]Attempts to impose complex budget formats (such as zero based budgeting), derived primarily for improving the delivery of products and services, fail when applied to the R&D realm. See H. A. Averch, Private Markets and Public Interventions: A Primer for Policy Designers (Pittsburgh, PA: University of Pittsburgh Press, 1990). A capital budget for large scale megaprojects of all types might improve decisions by forcing concrete tradeoffs.

NOTES ON THE ECONOMICS OF R&D AND PUBLIC POLICY, 1985-1990

H.A. Averch[1]

0. INTRODUCTION

Between 1985 and 1990, R&D economics continued to develop along lines and themes set by the seminal works of the 1950's and 1960's.[2] Theoretical results were deepened and made more complex and general. Early empirical results were tested on many different data sets. By and large, none of the early findings have been definitively rejected, although they have been refined. For example, most recent studies on the returns to R&D continue to find that they are high and, indeed higher, than most other forms of investment. However, much more attention is

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[2]For example, Arrow on relative incentives to innovate, K. Arrow, "Economic Welfare and the Allocation of Resources for Invention, in R.R. Nelson (ed.) The Rate and Direction of Inventive Activity (Princeton: Princeton University Press). Barzel on the timing of innovations, Y. Barzel, "Optimal timing Of Innovations, Review of Economics and Statistics, vol. 50, 1968, pp. 348-355. Griliches on the high returns to innovation, Z. Griliches, "Hybrid Corn: An Exploration of the Economics of Technical Change," Econometrica, vol. 25, 1957, pp. 501-522; Solow on the impact of technical change on overall productivity, R.M. Solow, " A Contribution to the Theory of Economic Growth, Quarterly Journal of Economics, vol 70, 1956, pp. 65-94; Denison on growth accounting, E. F. Denison, The Sources of Economic Growth in the United States and the Alternatives Before Us (Washington: DC: Brookings Institution, 19620; Mansfield on the determinants of industrial R&D, E. Mansfield, Industrial Research and Technological Innovation: An Econometric Analysis (New york,: W.W. Norton, 1968); Nelson on the public goods aspects of R&D, R.R. Nelson, "The Simple Economics of Basic Scientific Research," Journal of Political Economy, vol. 67, 1959, pp. 297-306; Schmookler on demand pull vs. supply push, J. Schmookler Invention and Economic Growth (Cambridge, MA; Harvard University Press, 1966). For a survey see, C. Kennedy and A. P. Thirlwall, "Surveys in Applied Economics: Technical Progress," Economic Journal, vol. 82, 1972, pp. 11-72 and M. Kamien and N. Schwartz, Market Structure and Innovation (Cambridge, UK: Cambridge University Press, 1982).

being paid to variations in returns across industries and to the industry- and firm-specific reasons for a given rate of return.[3]

The application of formal, noncooperative game theory to the economics of R&D has been perhaps the most striking new development in the 1980's.[4] However, the assumptions so far have been so restrictive, the results so unsurprising relative to the older literature, and so devoted to identifying equilibrium outcomes that any policy use or application would be highly imprudent.[5]

1. PURPOSE OF THIS PAPER

This paper briefly updates what we know about the economics of R&D and innovation from work in the 1985-1990 period. It is oriented toward public policy rather than technical economic theory. It proceeds by stating the issues and questions that have vexed policy makers, since the significance of R&D and technological change for economic growth and productivity was once more recognized by economists after World War II.[6]

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[3]G. Dosi, "Sources, Procedures, and Microeconomic Effects of Innovation," Journal of Economic Literature, vol. 26, 1988, pp. 1120-1171.

[4]See J. F. Reinganum, "The Timing of Innovation: Research, Development, and Diffusion," in R. Schmalensee and R. Willig (eds.), Handbook of Industrial Organization (Amsterdam: North-Holland, 1989).

[5]For example, this literature typically examines R&D and patent issues in the context of two firms postulated to behave symmetrically. Government as an active player never enters these game theoretic formulations.

[6]Classical economists like Adam Smith had no difficulty recognizing the importance of technical change. Nor did Marx. However, the neo-classical paradigm separated issues of static efficiency--making the best use of existing resources and products--and dynamic efficiency--making sure the economy could generate new products and processes. To this day, neo-classical economists (most of the profession) concentrate their research and teaching on static efficiency. Although Schumpeter concentrated on dynamic efficiency, it is only recently that formal, Schumpeterian models have appeared in the literature.

Then it gives the answers as they are understood today. Focus here is on the economics literature says between 1985-1990, using comprehensive surveys when possible.[7]

2. Productivity, R&D, and technical change

2. 1 Productivity growth is necessary, but not sufficient, for increases in domestic social welfare and international economic competitiveness. Technical change is a strong determinant of productivity, and the level and composition of R&D determines technical change. R&D and technical change are partly influenced by intended and unintended public policy.[8]

2.2 The productivity slowdown in the West since 1973[9]

Multiple causes account for the decline, but declines in R&D inputs explain only a small fraction of the decline in productivity across the

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[7]The reader should be aware that there is an enormous gap between the economics literature and the literature on R&D management. The management literature instructs managers of firms to invest in R&D and technological change, because they will create disequilibrium situations that will advantage the firm and, further, that enlightened R&D management can maintain and enhance these advantages. The economics literature suggests that managers are helpless in the face of an industry's structural conditions and the behavior of rivals. Since R&D and technical change destroy and create industries, the economic theory seems addressed to wrong, but tractable problems. To really address policy questions, economists would need a much more comprehensive theories of change than they have so far mustered. See W.N. Parker,

[8]A.N. Link, Technological Change and Productivity Growth. (New York: Harwood Academic Publishers, 1987).

[9]We now know that there was little productivity growth or no productivity growth in the Eastern block economies.

West, however measured.[10] Declines were industry specific, and high tech industry showed no decline.

2.3 R&D investments,diffusion, and productivity

R&D expenditures from any source can only have an impact if they result in the diffusion of new products and processes, and this takes time. Since downstream costs of diffusion are not counted on the cost side of R&D investments, standard computations of returns or of productivity of R&D may be overstated.[11] However, even when we include the true social costs of turning R&D into marketable products and processes, the computed returns on R&D or its marginal productivity should still be high.

3. Technical change and market structure

3.1 No market structure is superior in all situations with respect to the promotion of R&D and technical change. There are large and significant differences across firms and industries in production and use of R&D.

3.2 Since technological progress is highly important to social welfare, the perfectly competitive market structure which provides allocative and technical efficiency without technological progress is not

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[10]S. Englander and R. Evenson, "The Total Factor Productivity Slowdown", OECD Economic Studies, No. 11, 1988, pp. 8-42.

[11]P. Stoneman, The Economic Analysis of Technology Policy (Oxford, UK: Oxford University Press).

inherently superior to less competitive, but more innovative structures.[12],[13]

3.3 Some degree of market imperfection is necessary for technological change, but no one is able to specify the degree or how to achieve it.[14]

3.4 Technical change and antitrust policy

Until the 1980's, US. antitrust policy did not encourage innovation. Since 1980, Department of Justice and EOP interpretations have been more favorable.[15]

4. Patents, Optimal Patents, Research, and Monopoly

4.1 Pre-emptive patenting

The basic purpose of a patent is to increase the appropriability of inventions and so to provide an incentive to turn inventions into marketable innovations. provide. Under some conditions a firm may have an

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[12]W. L. Baldwin, Market Structure and Technological Change (New York: Harwood Academic Publishers, 1987).

[13]If intermediate microeconomics textbooks represent the collective rhetoric of the economics profession, then this view has yet to prevail. The amount of discussion of innovation in most micro texts is near zero except for passing reference to Schumpeter.

[14]See J. E. Stiglitz, "Theory of Competition, Incentives and Risk," in J. E. Stiglitz and G. F. Matheson, New Developments in the Analysis of Market Structure (Cambridge, MA: MIT Press), pp. 399-449.

[15]J. Langenfeld and D. Scheffman, "Innovation and U.S. Competition," The Antitrust Bulletin, vol. 34, 1989, pp. 1-64.

incentive to maintain monopoly power by patenting new technologies and letting them sleep, i.e., not using them.[16]

4.2 There is a tradeoff between a narrow patent of long duration and a broad patent of short duration.[17]

4.3 An innovation may supersede a patented technology. Patent design needs to consider directly the degree of novelty required to displace an existing patent. With a strong novelty requirement, minor incremental advances may not be patented.[18]

5. Standards

5.1 The unaided market works poorly to generate the degree of standardization required to maximize the benefits obtainable from related technological systems and components.[19] Government may want to take a hand in standard setting. Given the uncertainties in new technological developments, it is easy to settle on the wrong standard and be locked in.

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[16]J. Vickers, "Pre-emptive Patenting, Joint Ventures, and the Persistence of Oligopoly," International Journal of Industrial Organization, vol. 3, 1985, pp. 261-274.

[17]R. Gilbert and C. Shapiro, "Optimal Patent Length and Breadth," Rand Journal of Economics, vol. 21, 1990, pp.106-112; P. Klemperer, "How Broad Should the Scope of Patent Protection Be," Rand Journal of Economics, vol. 21, 1990, pp.113-129.

[18]S. Scotcmer and J. Green, "Novelty and Disclosure in Patent Law," Rand Journal of Economics, vol. 21, 1990, pp.131-146.

[19]P.A. David "Narrow Windows, Blind Giants and Angry Orphans," Technological Innovation Project Working Paper #19, Center for Economic Policy Research, 1986; M.L. Katz and C. Shapiro, "Network Externalities, Competition, and Compatibility," American Economic Review, vol. 75, 1985, pp. 424-40.

So any government intervention needs careful design with appropriate attention to flexibility.[20]

5 Defense R&D and civilian R&D

5.1 There is a strain of literature that argues that concentration on defense R&D is damaging to R&D on commercial products and processes and therefore damaging to growth and productivity. Recent literature also suggests that pursuing civilian technology via defense R&D is cost-ineffective because there is a growing divergence between performance specifications for military systems and those designed to meet market tests.[21]

5.2 The worth of military R&D for the civilian sector depends on the extent of "spin-off." The worth of spinoffs is problematic.

5.3 The sheer size of the defense procurement budget, and the consequent size of the defense procurement market, suggests that privately financed R&D may be distorted away from civil sector R&D.[22]

6. Tax and credit incentives for private sector R&D

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[20]R. Cowan, "Nuclear Power Reactors: A Study in Technological Lock-in, Journal of Economic History, vol. 50, 1990, pp. 541-567.

[21]S. Chan, "The Impact of Defence Spending on Economic Performance: A Survey of Evidence and Problems," Orbis, vol. 29, 1985, pp. 403-434.

[22]N. Rosenberg, "Civilian 'Spillovers' from Military R&D Spending: The American Experience since World War II, Conference on Technical Cooperation and International Competitiveness," Lucca, Italy.

6.1 For the particular design the U.S. used in the 1980's--a tax credit for incremental R&D--there was only a very modest positive outcome insufficient to overcome government revenue losses.[23]

[23]E. Mansfield, "The R&D Tax Credit and Other Technology Policy Issues," American Economic Review, vol. 76, 1986, pp. 190-194.